FOCUSED DISCUSSION: Scientific Expertise (invited paper)

Science Democratised = Expertise Decommissioned

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Science and expertise have been antithetical forms of knowledge in both the ancient and the modern world, but they appear identical in today's postmodern world, especially in the Science & Technology Studies (STS) literature. The ancient Athenians associated science (*epistemé*) with the contemplative life afforded to those who lived from inherited wealth. Expertise (*techné*) was for those lacking property, and hence citizenship. Such people were regularly forced to justify their usefulness to Athenian society. Some foreign merchants, collectively demonised in Plato's *Dialogues* as 'sophists', appeared so insulting to citizen Socrates, because they dared to alienate aspects of this leisured existence (e.g. the capacity for articulate reasoning) and repackage them as techniques that might be purchased on demand from an expert – that is, a sophist. In effect, the sophists cleverly tried to universalise their own alien status, taking full advantage of the strong analogy that Athenians saw between the governance of the self and the polis. Unfortunately, Plato, the original spin doctor, immortalised Socrates' laboured and hyperbolic rearguard response to these sly and partially successful attempts at dislodging hereditary privilege.

In any case, science and expertise led a more harmonious existence in the pre-modern Christian era, as everyone was expected to live by the sweat of their brow, an aspect of the labour theory of value that joined Thomas Aquinas to Karl Marx. Medieval monasteries were the original communes, in which the monks alternated between contemplating God and taking turns at the scriptorium and/or the vineyard. A privatised version of this ethic came to be known as the 'Renaissance Man', as exemplified by the careers of Leonardo da Vinci and especially Galileo. In this context, the boundary separating science and expertise became more porous, specifically enabling technical arts of instrumentation and experimentation to become constitutive of scientific inquiry itself. The Royal Society of London famously institutionalised that attitude.

However, the unprecedented achievement of Newtonian mechanics led

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many Enlightenment thinkers to conclude that science had nearly reached the limits of human comprehension, such that our ingenuity is best spent on making the most of this knowledge through applications that ameliorate the human condition and extend our dominion over the earth. The Greek attitude had been turned on its head: instead of science being the luxury of those who did not need to live by their expertise, expertise came to be seen as a political imperative to make the most of the virtually completed body of scientific knowledge. This shift in attitude was perhaps clearest in the case of mathematics, as championed by Diderot's co-editor of *L'Encyclopédie*, Jean d'Alembert, who regarded his discipline as an adjunct of engineering and political economy, and a statistically driven search for tolerable error in socially relevant contexts – not a Cartesian quest for superhuman certainty. The uncertainty of statistics was tolerable precisely because science, at least to the *philosophes*' satisfaction, had replaced theology as the foundation of knowledge.

This view of science persists in the legal incentives that modern states provide for inventors to turn the 'laws of nature' to their advantage – not least in the Enlightenment's most enduring political legacy, the United States' Constitution, which names patenting as a civil right. A society that took seriously how wrong we might turn out to be about the laws of nature – that science is, in Karl Popper's phrase, an 'unended quest" – would never have created a special category of 'patents' that confers a privilege on invention beyond what can be fetched on the open market. Instead an invention would be treated as an ordinary good possessing only exchange value, not some deeper value from one's having worked over a parcel of common reality, or "'intellectual property'. It follows that the sales registered for the invention prior to its market replacement would be sufficient reward, with no further need to grant the inventor some additional legal protection simply because he brought the idea to market first.

The nineteenth century witnessed the reinvention of the university as the institutional seat of science and the guarantor of expertise. This development followed a 'secular' and a 'sacred' course, the former traceable through Wilhelm von Humboldt's promotion of philosophy as the synthetic discipline of citizen education, the latter through William Whewell's promotion of Newtonian mechanics as theology's scientific face, which justified all other theoretical and practical pursuits. Where Humboldt wanted a curriculum that would shift student allegiance from the church to the state, Whewell aimed to ensure that the church remained relevant to a rapidly secularising economy. In both cases, the university would 'internalise the externalities' of a society that encouraged innovation without having anticipated its long term consequences: Expertise could not be based simply on the personal testimony of either the producers or the consumers of a purveyed good or technique. Rather, expertise must be underwritten by scientific principles, which the reinvented universities would be in the business of nominating, organizing, testing and promulgating.

In the final quarter of the twentieth century, Jean-François Lyotard (1983)

fashioned the phrase 'the postmodern condition' to capture the 'always already' doomed character of the university's mission. Here Lyotard made an invidious but persuasive comparison with the mythically 'progressive' status that socialism had acquired, albeit sometimes by violent means, over roughly the same two centuries. For Lyotard 'science' (understood as a unified body of knowledge instantiated in the university) and 'society' (understood as a unified body of action instantiated in the state) were fictions that had outlived whatever usefulness they ever had in bounding developments whose very nature exceeded all attempts at bounding. Lyotard argued the point in largely empirical terms, observing just how much intellectual innovation in the recent past (e.g. computer science, molecular biology) occurred off-campus in heterogeneous research teams lacking any obvious disciplinary home. He concluded that what universities continue to mystify as 'science' (cf. states continued to mystify as 'society' or better still, 'welfare') is really the product of locally developed expertises, which universities only later exploited to their own advantage.

Lyotard drove a stake into the heart of any project that drew sustenance from the Enlightenment legacy. STS has been probably the main beneficiary, notwithstanding Latour (1993), which 'doth protest too much' in trying to distance STS's 'non-modernism' from Lyotard's postmodernism. Lyotard's anti-university vision may be compared with my own pro-university vision in terms of an analogy drawn from political economy (cf. Fuller 2007). On the one hand, Lyotard sees the university as the appropriator of surplus value from the truly creative researchers and inventors who work in places kept apart – both conceptually and physically – from the university's inner sanctum, the classroom and the curriculum committee. On the other hand, I see the university as a vehicle of 'epistemic justice', precisely through its educational function, which effectively redistributes knowledge-based advantage from the elite clients who are the primary beneficiaries of innovation to a student audience that has historically encompassed a broader range of backgrounds and interests. In short, whereas Lyotard sees universities as commissioning expertise by granting it epistemic authority, I see them as decommissioning it by spreading that authority widely.

Over the past quarter century, analytic philosophy has moved in a parallel direction, though largely without acknowledging the corresponding world-historic trends. Indeed, analytic philosophy's two main conceptions of expertise are rarely distinguished, let alone perceived in mutual tension. One is Hilary Putnam's (1979) 'linguistic division of labour', the other Philip Kitcher's (1993) ' division of cognitive labour'. While trading on the sociological idea of 'division of labour', they nevertheless divide the relevant labour rather differently. Putnam's point is that speakers normally know what they mean from the context of usage, except for 'hard cases' that require experts who spend their time studying what distinguishes *p* from ~*p*. Implied here is a theory of expertise that would tell us to

seek a physician only when we cannot manage our bodies by the usual means. In contrast, Kitcher's point is that reality is carved up into discrete expertises, such that our claims to know something are always already accountable to those who spend their time studying it. Implied here is a theory of expertise that would tell us to seek a physician on a regular basis, since *prima facie* the physician knows our body better than we do. Of course, in most cases, our own and our physician's judgement might converge – but the convergence matters, at least epistemologically.

What distinguishes Putnam's and Kitcher's positions? The difference here clearly matters for those who worry that things are done for the right reasons. However, if all that concerns us is that the right things are done, regardless of reasons, then Putnam and Kitcher merely chart alternative routes to destinations that will coincide in the vast majority of cases. The sociology of knowledge gives us some initial insight into this matter, since Putnam (born 1926) and Kitcher (born 1947) belong to different generational cohorts. Putnam writes when Marxism was most respectable in Anglo-American academia (and Putnam himself would drop quotes from Mao and Althusser), while Kitcher writes in a post-Marxist, neo-liberal world (which does not think twice about using neo-classical economics to model the science system). Putnam's view presumes that we are epistemic equals unless shown otherwise, while Kitcher's presumes the exact opposite. Behind these presumptions are opposed social-epistemological worldviews that provide alternative answers to the question: Does our status as competent members of society *ipso facto* underwrite our epistemic authority? Putnam says yes, Kitcher no.

For his part, Putnam takes seriously that everyone enjoys equal access to reality. When people disagree, that is simply because they have different evidence at their disposal or weigh the same evidence differently, all of which is tractable to negotiations with other people who are in the same epistemic state. Call this the 'primitive communist' approach to social epistemology. It implies that the need for expertise is limited to 'technical matters', where an unusually prolonged focus on a specific topic serves to resolve uncertainty and disagreement. Although the Athenians held a notoriously elite view of citizenship, their attitude toward expertise was very much in this vein: mere *techné*. Thus, Plato and Aristotle praised expert craftsmanship for its capacity to realize in matter an idea that would otherwise remain inchoate in the client's mind. But there is no sense that the craftsman is either the source of the idea or the ultimate arbiter of its realization.

The conversion of *techné* to bureaucracy – from commercial trade to civil service – is a signature theme in modern German philosophy, starting with Humboldt, Fichte and Hegel. It is how the Athenian attitude came to be democratised. The German idea was to incorporate more people as epistemic equals through a proactive state-based educational system, with expertise relegated to increasingly detailed and potentially routinised administrative tasks. When Marx and Engels spoke about the 'withering away of the state' under

Communism, they were refashioning a phrase Fichte had used to chart this trajectory. Indeed, Marx and Engels saw the party carrying on the work of the university as expedited by the industrial development of labour-saving technology – provided that the social relations of production were wrested from capitalist control. In the resulting communist utopia, expertise would be on tap – in a black box? – to remove the drudgery as we explore the multifarious aspects of our humanity.

Before turning to Kitcher's rather different attitude towards expertise, two remarks are in order about Putnam's social-epistemological vision. First, I believe that, despite its empirical failure and unfashionable status, this vision takes seriously the fullness of our humanity. Its revival will not be easy, however, and the tenor of STS research goes largely against it. But 'utopian' here should be interpreted to mean 'difficult' or 'against the grain' not 'impossible', let alone 'wrong'. Second, seen as a historically unfolding idea, this vision reveals the underrated appeal of an 'instrumentalist' philosophy of science and even the 'instrumentalisation' of scientific practice. These notions presuppose that humans supply the ends on whose behalf those 'instruments' would be deployed. By not building ends into the instruments themselves – that is, by denying that science as such or its constitutive practices have ends of their own – we as humans are given a potentially free hand to fashion the ends for ourselves. I say 'potentially', of course, because the question of the 'ends of science' gets shifted from something about how science intrinsically works to who has the right and power to deploy the relevant instruments without interruption. To be sure, the question of who 'we' are remains subject to contestation – but no less so than the question of what it is about a practice that makes it scientific. However, the same question posed in political terms focuses the mind – and action – in a way that it does not when posed in metaphysical terms: The former is about what it takes to be free, the latter about what it takes to be determined.

For his part, Kitcher's conception of expertise is proprietarian, an extension of John Locke's version of the labour theory of value. No one can lay claim authoritative claim over a domain of reality, even the reality of one's own body, until they have worked it over with intensive study. For Locke, this position constituted, on the one hand, a criticism of the casual instrumentalisation of persons allowed by the law of slavery and, on the other, an endorsement of the Protestant idea that persons are obliged to undergo the self-study associated with the cultivation of conscience and the adoption of discipline. The former removed an arbitrary royal privilege, while the latter constituted modes of enquiry that the Protestants had wrenched back from the pastoral mission of the Catholic clergy and, to a lesser extent, secular medicine. Locke, a physician notoriously intolerant towards Catholics, took the 'empiric' – i.e. sceptical – view that allopathic intervention should be permitted under extreme circumstances

after several physicians had been consulted. Such was the model for Locke's legislative prerogative over either royal edict or personal judgement in a just society (Romanell 1984).

There was a period from, say, 1700 to 1900, when the religious and scientific senses of 'discipline' vis-à-vis the human body were largely the same. This period coincides with the secularisation of conscience as consciousness, and the ascendancy of 'introspection' as a putatively reliable mode of epistemic access. However, the route from Locke to Kitcher starts to get paved in the second half of the nineteenth century, when a scientifically reinvigorated medical profession, including psychiatry, provided new secular grounds for claims to expertise over personal space previously held (and by then largely abandoned) by the pastoral clergy. As a result, we now routinely defer to the advice of physicians without equating it with the fear of God or the demands of slave masters. This is because we naturally approach our own bodies less as seats of agency, let alone sovereign power, than as sites of investigation that are *terra incognita* until staked out by those who have undergone proper training, which typically involves restricting, if not undoing, the lessons of personal life experience. In that respect, we are all already 'patients'. This is the epistemic version of the social contract to which lay people and experts agree in Kitcher's division of cognitive labour. It results in the familiar image of the history of science as the colonisation of what the later Husserl called the 'life-world'. In our times, the lawyer Peter Drahos (1995) has observed the emergence of a second-order version of the same tendency in cyberspace under the rubric of 'information feudalism'.

Kitcher's social-epistemological vision is also one with which STS is largely – and regrettably – comfortable. The origins of this attitude lie in issues associated with the most influential school of sociology in early STS research: ethnomethodology. Over thirty years ago, ethnomethodologists had raised the question of knowledge 'ownership', partly in response to a perennial problem in the politics of ethnography that had come to a head in the heightened academic consciousness of the 1960s: To what extent is the analyst accountable to the analysed? This problem arises because an ethnographer's subjects are potentially subject to the designs of her clients in government or business who have a vested interest in understanding the movements of natives, deviants, and other key target groups. Does a good ethnographer in the name of 'giving voice' to these groups end up betraying whatever secrets had enabled them to elude more powerful forces in society? The fact that even today cooperative subjects are called 'informants' suggests that the problem has not been fully solved. In a heated debate with Howard Becker at the US Society for the Study of Social Problems in the 1960s, Alvin Gouldner accused ethnographers of illicitly appropriating the knowledge of vulnerable groups, effectively placing them at risk, while presenting themselves as champions of dispossessed countercultures (Fuller 2000: 363). (Were Gouldner alive today, he would probably make a similar argument against medical anthropologists who work for pharmaceutical

industries on bioprospecting projects.)

Against this critical backdrop, ethnomethodologists provided a self-protective scholastic response (e.g. Sharrock 1974). They identified the possession of knowledge with the production of accounts of knowledge. Insofar as the accounts of the analyst and the analysed are produced in different contexts, in different words and for different ends (which may or may not be achieved), they are different pieces of knowledge, each owned by their respective producer, as a labour theory of value would have it. Ethnomethodologists were especially well-placed epistemologically to make this argument. They broke with traditional ethnography on two crucial points relating to their radical social constructivism. First, ethnomethodologists upheld a minimalist view of knowledge as whatever passes for knowledge in a particular social context, without presuming, say, the prior existence of cognitive traditions, unless they are conjured up (discursively) in that context. Second, ethnomethodologists were notorious for their strategic interventions in ongoing social practices, very much in the spirit of experimentation, which deliberately undermined any notion that their accounts 'mirrored' or even 'represented' the subjects analysed.

At first STS seemed to adopt the ethnomethodologist's pose towards knowledge production unproblematically. The field made a persuasive case that it had staked out its own distinct domain of knowledge that drew on agents' first-order experiences but presented them in a fashion that was at once alien from yet illuminating to those agents. The exemplar of this moment is Jonas Salk's preface to Latour and Woolgar's *Laboratory Life* (1986), whose laboratory in San Diego provided the site for what remains *the* classic STS ethnography. While the language of *Laboratory Life* was hardly obscurantist by the standards of the late 1970s – the period when Foucault and Derrida were translated into English – it was nevertheless sufficiently indebted to discourses unfamiliar to either their subjects or those who might be interested in their subjects' activities to carry a strong sense of autonomy and integrity.

At the same time, however, the excitement surrounding early STS fed off the frisson of radical critique associated with the rhetoric of 'alienation', which tapped into the rediscovery of the 'young' or 'humanist' Karl Marx, whose unpublished manuscripts were translated into English in the 1960s. This Marx tended to treat social, including economic, structures as alienated ideological formations – 'reifications', to recall Gyorgy Lukacs' term – abstracted from concrete practices, or 'praxes'. The bellwether text was Berger and Luckmann's *The Social Construction of Reality* (1967), which continues to be fondly cited by STSers of the '68 generation regardless of their current politics. For a fleeting moment, the cunning of reason greeted the invisible hand: Cold War polarities appeared to self-deconstruct once Marx was revealed to have been an avid reader of Adam Smith before the latter became a capitalist icon. However, this

early flirtation with Marxism came back to haunt STS after the collapse of Communism and the onset of the Science Wars. These two events are connected by science's loss of default generous national funding, once the Soviet Union was no longer seen as a substantial high-tech security threat. In this shifted context, talk of 'material practices' appeared to turn science into an activity whose own practitioners were its primary and perhaps sole beneficiaries. Here STS suggested that work done outside the laboratories was required for work done inside them to acquire scientific status. If so, shouldn't scientists themselves – rather than an already overloaded state – bear the burden of recruiting allies to advance any research programmes? Intentionally or not, STS promoted the idea that science had to be justified not in some general, long-term, collective sense but in terms of specific, short-term, constituency-based horizons: a shift from a state to a market vision of science.

For STS to evolve into a kind of 'meta-scientific' expertise, an increasing proportion of those competent in the field should enter 'science policy', broadly defined, to orchestrate this transition in the mode of science's societal justification. In fact, this has probably already happened. However, in practice, such people have effectively abandoned STS's research arena to an academically based community that has moved in the exact opposite direction. For those steeped in STS, this schism is exemplified in the contrasting trajectories of Bruno Latour (who articulates the ideology of policy-making STS) and Harry Collins (who articulates the ideology of academic STS), neither of which from my own standpoint is satisfactory.

Suppose we ask the pointed question: Who won the Science Wars – 'them' or 'us' (where 'they' are the scientists and 'we' the STSers)? From the standpoint of the normative criteria used in contemporary science policymaking, 'we' seem to have emerged victorious. Whenever a funding agency evaluates a grant proposal in terms of the 'users and beneficiaries' from outside the peer scientific reference group, STS expertise is vindicated. Yet at the same time academic STS has increasingly cast its own expertise as simulating, if not approximating, the expertise of first-order science. Thus, Collins and Evans (2002) plot the history of STS as progressing through three stages: no expertise, interactional expertise and contributory expertise. Accordingly, STS charts its success by how much its researchers can contribute substantively to the projects of the scientists they study.

Yet this narrative comes close to saying that the task for STS researchers is to reinvent by exclusively sociological means the sorts of skills that science pedagogy normally – and more efficiently – provides. But why should sociologists interested in acquiring 'contributory expertise' in a science not instead simply acquire a degree in the science? It would certainly be quicker than picking up the relevant knowledge by osmosis over many years by hanging around the relevant scientists. Indeed, for a field like the study of gravitational waves, it would probably result in a more streamlined presentation than the 864-paged Collins (2004). Under the circumstances, the take-home lesson of 'contributory

expertise' for *STS* as an autonomous body of knowledge remains obscure if it is not denied altogether. 'Contributory expertise' is an unequivocally *progressive* moment in the history of STS only if the final court of appeal for the value of STS research are the scientists whom STS studies. In that respect, 'they' won the Science Wars.

And while Collins may have the most developed record of research in the STS study of expertise, his general orientation to expertise is implicit in how STS judges its own work. I belong to the first generation of people trained in the STS fields who were told that our intellectual credibility would be enhanced by mastering the science of which one would do the history, philosophy or sociology. Whatever one now makes of this advice (which I didn't take), it strongly suggested that STS research could only be as good as the mastery of the studied science that it displayed. Whatever distinctive slant or perspective STS provided was in addition to, and presumably detachable from, the show of scientific competence. Consequently, the least controversially excellent work in STS is by people – say, Donald MacKenzie and Peter Galison – whose intellectual calling card is technical virtuosity presented with a light theoretical touch. Given MacKenzie's succession of research topics – statistical controversies in genetics, accuracy in military weapons, the computerisation of mathematical proofs and the modelling of financial markets – it probably comes as no surprise that he began his academic career with a first class honours degree in applied mathematics from the University of Edinburgh. Indeed, if an overall pedagogical lesson is to be gleaned from MacKenzie's career, it is that very little sociology goes a very long way, if the STS researcher already possesses a first-hand understanding of the science she studies. For his part, Galison bypassed the circumambulations of Collins' 'contributory expertise' by going native and acquiring a physics Ph.D. alongside his doctorate in the history of science. He sees 'theory' as providing shade and nuance to locally constrained practices, much in the manner of an artist whose technique compensates for potential deficiencies in the observer's perspective on an object (Galison 2004). What theory does *not* do is to place the object in a radically different light, potentially subjecting it to criticism.

Here it is worth recalling that the prehistory of STS consisted of people who approached matters from quite the other way around: They were already expert in the natural sciences and mathematics but they wanted to distance the nobler concerns of their disciplines from their secular entanglements in World War I, World War II and the Cold War. (I mean here to cover everyone from Rudolf Carnap to Barry Barnes.) That aim forced them to move into history, philosophy and sociology, disciplines that still allowed the expression of rapidly disappearing, if not entirely lost, normative ideals. To be sure the ideals promoted by, say, Carnap and Popper, Kuhn and Lakatos, Barnes and Bloor varied in detail. Nevertheless, they had a shared sense of the task – namely, to

justify science by 'natural philosophical' standards that Newton would have recognised as his own. However, these were not necessarily the standards to which most scientists in the twentieth century have aspired, let alone realized. Indeed, the prehistory of STS can be read as an invocation of the past to criticise contemporary science for being too fragmented, instrumentalised and otherwise fallen.

All of this stands in striking contrast with the decidedly 'anti-critical' stance of STS vis-à-vis science in the wake of *both* Latour and Collins. For example, when MacKenzie (2006) writes of the 'performative' character of economic models of financial markets, he is more concerned with how models succeed in shaping markets than with whatever power the models exert as critical forces, especially when they *fail* to shape markets. Yet the epistemic authority of economics, like that of medicine, is evidenced more in the guilt that society feels for failing to live up to its normative ideals than in the ease with which it can make society conform to its explanatory ideals. (Consider attitudes towards inflation and obesity: rarely managed but always regretted.) To be sure, in both cases the same models are at play but they are seen in rather different lights – specifically, in terms of what might be called the 'vector of accountability': Are economists ultimately accountable to the markets they help bring into being, or are markets accountable to economists, whose criticism renders markets problematic in ways that demand a concerted social response? Economists in both cases may get their way, but it is only in the latter case that their expertise counts as an independent force countermanding other, locally based and typically elite, expertises.

Let me conclude by drawing together the various strands of my argument. Science and expertise are historically opposed ideas: The former evokes a universalistic ideal pursued in leisure, while the latter consists of particular practices pursued to earn a living. However, expertise can serve the universalistic ideal of science by undermining the authority of other expertises that would cast doubt on the viability of this ideal. Put bluntly, expertise is 'progressive' only when it serves as the second moment of a Hegelian dialectic. *Contra* Lyotard and most STS treatments of science, I see the modern university – specifically through its teaching function -- as the place where this moment most often happens. STS has failed to recognize that the project of 'democratising knowledge' ultimately means that expertise is not to be conserved but actively decommissioned. It follows that what is still often valorised in STS circles as the 'tacit' or 'craft' character of expertise should be critiqued as a mystified version of what economists call 'path dependency' – that is, in philosophical terms, an attempt by those who originated a robust body of knowledge to conflate the contexts of discovery and justification to maintain their initial advantage. The challenge here to STS can be posed as an explicit research imperative: If we remain committed to the democratisation of knowledge, we should always try to find some less costly alternative path to the modes of thought and action currently licensed by a given expertise – and then ask why that cheaper route is

not already dominant. This drive towards intellectual efficiency includes rendering esoteric research pedagogically tractable, transferring skills from humans to machines, converting virtuosity into routine and reassigning the significance of the division of labour from its role in Kitcher to that in Putnam. It also means restoring *breadth* to its rightful place ahead of *depth* as a value in knowledge.

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